

# SCIENCE

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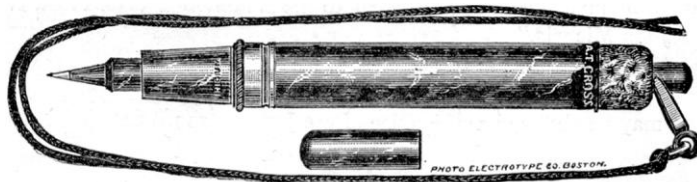
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JOHN MICHELS, Editor.

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## SWINE PLAGUE.

The present discussion on the infectious disease existing among hogs in the United States, known as the "*Swine Plague*" will, we trust, be productive of some good in giving publicity to certain facts relating to this subject, which should be known and understood by all interested in the sale or consumption of pork.

It appears that a report was sent from the British Consulate at Philadelphia, to England, stating that 700,000 hogs had died of Swine Plague during the year 1880, in one of the Western States.

Those interested in the export trade have contested this statement, and with the very laudable motive of protecting an important American home interest, have endeavored to show that the action of the Consul was founded on erroneous information, and one journal in New York even accuses the British officials of "plotting a senseless scare."

It appears to be now officially admitted that 300,000 hogs died of this disease in one State alone in 1880; and, therefore, the real question now at issue, is not whether the disease exists, but merely how many hundred thousand hogs die in consequence of it annually in each State.

Without going outside of United States official documents the real facts of the case may be stated as follows:

The Swine Plague came into notice about 25 years ago, and on account of its excessive infectious nature, it steadily increased annually until the year 1878, when the Commissioner of Agriculture announced an annual death-rate of hogs for the United States, amounting to a money value of \$20,000,000; as the victims are said to be chiefly among the smaller and leaner animals, probably \$2 per head would be a fair average of value; in that case the number of deaths among hogs by the Swine Plague, actually taken by census, would be 10,000,000 for that year.

As this disease is no sudden epidemic, but has been progressing for a quarter of a century, it is not likely that, in the two years and a half which have passed since this report was made to the United States Government, the disease has much abated. The disease is at this date officially admitted to be raging, and the

mere question of its destructive effects, is only one of degree.

Under these circumstances it would appear unjust to accuse foreign consuls of partial conduct in reporting these facts, and it is equally futile to attempt to suppress them.

The behavior of the New York Produce Exchange in this matter reminds us of the action of the ostrich when it buries its head in the sand at the approach of danger. We have one word of advice to those who would preserve the United States export trade in pork, and that is to admit the existence of Swine Plague, and the increasing contamination of pork by trichinæ. This done, it is not difficult to organize such a system of inspection as will satisfy foreign governments that the shipments of pork from this country are such as can be received with safety. At the date of our writing, a cable dispatch announces that the Austrian Government has interdicted the importation of American pork in any form, and unless our suggestion is accepted without delay, other foreign States will probably follow the example of Austria on this question.

Major J. W. Powell succeeds Clarence King as Director of the United States Geological Survey. This appointment appears to have given general satisfaction, and we consider it a fortunate circumstance that a gentleman of such high professional attainments has accepted this important position.

## ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA.

A course of practical instruction in Invertebrate Paleontology, to be given under the auspices of the Academy of Natural Sciences of Philadelphia, was inaugurated by Professor Angelo Heilprin, on Tuesday, March 8th, 1881, at 8 P. M., in the Hall of the Academy.

The lectures, twenty-five in number, will be continued on the successive Fridays and Tuesdays of each week, from 4 to 5 o'clock, P. M.

The plan of instruction will embrace the examination of the life-histories of the various geological formations, the discussion of the biological relations of past organic forms, and the practical determination of these forms for the purposes of paleontological inquiry. The demonstrations will be of an essentially practical nature, and will be based upon a careful study of the resources of the Academy's collections.

A course of practical instruction in Mineralogy was also inaugurated by Professor Henry Carvill Lewis, at the Academy of Natural Sciences of Philadelphia, on Tuesday, March 15th, at 8 P. M. The lectures will be continued on successive Mondays and Thursdays at 4 P. M., beginning March 21st.

The course will consist of ten lectures, and will be in great part practical, and confined to Determinative Mineralogy. Blowpipe analysis, and the application of simple chemical tests to the determination of minerals, will be especially dwelt upon. Students will be expected to devote at least half the time to the performance of practical work in this department.

The course will also embrace a reference to Physical and Crystallographic Mineralogy, and to Mineralogical classification.

Application for admission to the above courses may be made to Henry McCook, Chairman of the Committee on Instruction and Lectures.

# A PARTIAL REVISION OF ANATOMICAL NOMENCLATURE, WITH ESPECIAL REFERENCE TO THAT OF THE BRAIN.\*

By BURT G. WILDER, M. D., Professor of Comparative Anatomy, etc., in Cornell University, and of Physiology in the Medical School of Maine.

## I. INTRODUCTORY.

During the preparation of a paper "On the Gross Anatomy of the Brain of the Domestic Cat (*Felis domestica*)," I have been led to believe that some advantage may be gained by certain modifications of the current anatomical nomenclature. The present article contains suggestions, chiefly of a practical nature, which I wish to submit to other anatomists in the hope that, even if the changes here indicated do not meet their approval, they will be induced to take the general subject into consideration.

That the nomenclature of a science is worthy of attention is indicated by the care bestowed upon the language of modern chemistry and mathematics, and by the following expressions of opinion:

"Everything in science ought to be real, ingenuous and open; every expression that indicates duplicity, or equivocation, reservation, wavering or inconsistency, is a reproach to it."—Barclay, A., 89†

"Questions of definition are of the very highest importance in philosophy, and they need to be watched accordingly." Duke of Argyll, 1.

"In all sciences, nomenclature is an object of importance; and each term should convey to the student a definite meaning." Dunglison, A, Preface.

"There is a necessity for perfect definiteness of language in all truly scientific work." P. G. Tait, 1.

"Technical terms are the tools of thought."‡

"Only an inferior hand persists in toiling with a clumsy instrument, when a better one lies within his reach."

"A single substantive term is a better instrument of thought than a paraphrase." Owen, A, 1, Preface, pp. xii, xiv.

"As morphology deals with forms and relations of position, it demands a careful selection of terms and a methodical nomenclature." Goodsir, A, 11, 83.

These remarks apply to the general subject of anatomical nomenclature. But the terms employed by anatomists form two divisions: those which indicate the *position* or *direction* of organs, and those by which the organs themselves are designated. Since, also, writers have usually treated of them separately, it will be convenient here to consider anatomical *toponomy* and *organonomy* under distinct headings.

## TERMS OF POSITION AND DIRECTION—TOPONOMY.

Dr. Barclay's volume had especial reference to this division of the subject, and its key-note is struck in the following paragraph (A, 5):

\* This article is based upon two communications: the one, "A Partial Revision of the Nomenclature of the Brain," was read at the Boston meeting of the American Association for the Advancement of Science, August 28, 1880, and was reported, in part, in the *Boston Daily Advertiser*, of August 30, and in the *New York Medical Record* for September 18th, 1880; the other, "On some Points of Anatomical Nomenclature," was read at a meeting of the Cornell Philosophical Society, Ithaca, N. Y., January 15, 1881.

† In the List of Works and Papers at the end of this article, the names of the authors are placed in alphabetical order. The titles of separate works are designated by letters, and their order has no significance: The titles of papers are numbered. In the case of papers published between 1800 and 1873 the numbers correspond to those in the chronological "Catalogue of Scientific Papers published by the Royal Society of London." In other cases the numbers are only provisional, and are printed in italics.

The references are made as follows: the name of the author is given first, unless the author has been indicated already; then follows the letter or the number by which the title of the work or paper is designated upon the list; if a Roman numeral is given it denotes the number of the volume; and the last number is that of the page. This system of references was followed by me first in 1872, in the paper entitled *Intermembral Homologies* (10), and has been since adopted by others.

‡ I have mislaid the reference to the source of this aphorism. Perhaps some of my readers can supply it.

"The vague ambiguity of such terms as superior, inferior, anterior, posterior, &c., must have been felt and acknowledged by every person the least versant with anatomical description."

Dunglison admits (A, 61) that "Great confusion has prevailed with anatomists in the use of the terms before, behind, &c." Dr. Spitzka has forcibly stated (1, 75, note 1) the objections to the use of anterior, &c., and their unsuitability is tacitly conceded in the employment of other terms by several writers who do not explicitly condemn the current toponomy: Gegenbaur (A, 491), Mivart (A, 69), Cleland (1, 170), Rolleston (B, 33, note), &c.

Finally, the need of a radical change of base has been proclaimed in one of the very strongholds of anthropotomy:

"Now that the more extended study of comparative anatomy and embryonic development is largely applied to the elucidation of the human structure, it is very desirable that descriptive terms should be sought which may, without ambiguity, indicate position and relation in the organism at once in man and animals. Such terms as cephalic and caudal, dorsal and ventral, &c., are of this kind, and ought, whenever this may be done consistently with sufficient clearness of description, to take the place of those which are only applicable to the peculiar attitude of the human body."—Quain, A, 1, 6.

This is certainly explicit as to the principle involved, and it is to be hoped that later editions of this standard *Human Anatomy* may display its practical application to the body of the work.

How slender is the justification for retaining a toponomical vocabulary based upon the relations of organisms to the surface of the earth, appears more fully when we reflect that the assumed standard, for the higher vertebrates at least, is man in his natural erect attitude; yet that both man and animals are more often examined and compared when lying upon the *back*, this being an attitude truly characteristic of only that infrequent "subject," the sloth.

As a single illustration of the logical inconsistencies into which we are led by the use of the current toponomy, let us take the series of possible designations of the direction of some vertebral spinous process which projects toward the skin of the back at, or approximately at, a right angle with the myelon. With man the direction in which it points is *posterior*, but with a cat it is *superior*, while with an ape or a bird it is somewhere between the two; with all four, when on the dissecting table, it would be usually *inferior*. Finally, with a flounder the corresponding direction would be *horizontal* or *sidewise*.

In short, to designate the locations of organs by the relation of animals to the surface of the earth, which relation differs in nearly allied forms, and varies with the same individual according to circumstances, is as far from philosophical as it would be to define the place of a house or a tree by reference to the planet Jupiter, or to assume that mankind naturally face the rising sun, and hence to designate our right and left as the south and north sides of the body.

Some practical points respecting this division of the subject will be presented farther on.

## DESIGNATION OF ORGANS,—ORGANONOMY.

There are probably few investigators or teachers of comparative anatomy who have not been impressed, in some degree, with the desirability of some modification of the prevailing nomenclature of organs,—the "bizarre nomenclature of anthropotomy," (Owen, A, 11, 143)—based as it is upon the peculiar features of the human body, which has been fitly characterized, from a morphological point of view, as "not a model, but a monstrosity."

This impression may give rise to special papers, like those of Owen, (166), MacIise (1), and Pye-Smith (1, 16), or simply to more or less extended remarks upon the subject, with or without the use or presentation of new terms.

In the Preface to his "Anatomie du Chat" (A, pp. xiv—xvii), Straus-Durckheim devotes several pages to a discussion of anatomical nomenclature, and the body of the work contains many original names. Professor H. S. Williams calls attention (A, Preface), to the "crying need of a standard and uniform nomenclature of comparative anatomy."

In the preface to their recent account of the morphology of the skull (A), Parker and Bettany say: "It has been attempted to narrate the facts by means of a consistent terminology, amplifying what Prof. Huxley has so admirably developed." Several of Huxley's papers (as 70), contain new terms, most of which have been generally accepted, and in a greater or less degree the same is true of the elder Agassiz (A), Gegenbaur (59), Hæckel (A), Marsh (1), and others.

That my own consideration of the subject is not wholly of recent date may be seen from the papers numbered 10 and 2.

#### SCOPE AND METHODS OF THIS REVISION.

Most of the toponomical terms here discussed have a general application. But a revision of the organonomy of the entire body would extend this article beyond desirable limits.

As stated by Pye-Smith (1, 162), "the nomenclature of the brain stands more in need of revision than that of any other part," and on the present occasion I will simply endeavor to remove, in some degree, the deficiency implied in the following words of the French editors of "Huguenin" (A, Preface):

"That which is demanded of anatomy is an exact nomenclature and determination of the parts of the brain in their relative positions and contiguity, and if possible in their continuity."

Doubtless, for the entire comprehension of its functions, and even for the final determination of some of its homologies, the vertebrate brain should be fully understood in respect to the disposition of its cellular and fibrous elements,—that which the writers just mentioned term its *continuity*. But whoever is at all familiar with the literature of encephalic histology, or who has undertaken for himself the exhaustive study of even a very limited part of the brain will, if of sincere mind, admit the present impossibility of fairly discussing the microscopical terminology of the organ within the limits of a single article.

With the gross anatomy of the brain, the case is somewhat different. In the first place, some knowledge of it is requisite as a foundation for the histological enquiry, as well as for general work in human or comparative anatomy, physiology, and pathology. Secondly, the parts which are distinguishable by the naked eye are comparatively few, and while the numerous errors which may be found in even standard works sufficiently attest the difficulties of encephalotomy, its methods are comparatively simple. It is to be hoped, however, that the microscopical terminology and synonymy of the brain may shortly find due treatment.

A recent paper is entitled by its authors: "A Reformed System of Terminology, etc." Now the word *reform* is generally associated with questions of ethical improvement; whereas terminological reforms involve no other principle than that of expediency, taking into the account, however, the future as well as the present and the past. Such moral truisms as "do right because it is right" have no counterparts in considerations of scientific nomenclature, and he who, affected by the *cacoethes reformandi*, insists upon reform for the sake of an ideal perfection, is apt to appear as nothing better than a troublesome and useless pedant.

In the place, then, of what otherwise might be styled the principles of terminological reform, I will enumerate briefly the objects of the present revision, the consider-

ations upon which it is based, and the methods which have been pursued:—

To facilitate the acquisition and communication of accurate anatomical knowledge, by rendering the vocabulary equally applicable to all vertebrates, and equally intelligible to all nations.

That the test of the accuracy and completeness of a description is, not that it may assist, but that it cannot mislead.

To include in this vocabulary, so far as practicable, only such terms as are brief, simple, significant, of classical origin, and capable of inflection.

To propose as few changes as possible, and to introduce new names only for parts apparently unknown or unnamed before (*e. g.*, *crista fornicis*), or in the place of semi-descriptive appellations undesirably long or incapable of inflection, as *e. g.*, *cimbria for tractus transversus pedunculi, porta for foramen Monroi*.

To consider *brevity* as an especially desirable characteristic of such names as are most frequently employed.

When a part is known by a descriptive phrase, to select therefrom some characteristic word as the technical designation; *e. g.*, *iter (a tertio ad ventriculū quantum)*.

When two or more parts are similar, or have similar relations, to distinguish them by joining to some common title already in use, prefixes indicative of their relative positions; *e. g.*, *postgeniculatum, prageniculatum*.

To shorten the names of several parts by omitting the word *corpus*, and using the neuter adjective as a substantive.

To keep modern usage, and the rules of classical etymology constantly in mind, but not to be hindered thereby from the employment or even the formation of terms which are eminently desirable from the practical standpoint.

To discard terms which indicate *size*, those which refer to the *natural attitude* of man or animals, most *vernacular* names, and all names of the reproductive organs which have been applied needlessly to other parts of the body.

With regard to the point last-named, while it may perhaps be urged in extenuation that the *patres anatomici* entertained a notion as to the representation of the entire organism in the brain, some of their words certainly indicate an entire freedom from apprehension that the mysteries of encephalic anatomy ever would be discussed by ordinary mortals, much less by women, or under circumstances requiring propriety of speech.

As has been stated, and as will be exemplified in the vocabulary, I have placed great stress upon *brevity* as a desirable characteristic of anatomical terms. So long as the study of anatomy was nearly confined to members of the medical profession, they being comparatively few in number, and, by ancient tradition at least, not wholly averse to clothing their discourse in a sesquipedalian garb impenetrable to the vulgar eye, it mattered little whether the statement of a given fact or idea required one minute or five. But now, thanks to the popular writings of Agassiz, Dana, Gray, Darwin, Hæckel, Huxley, Owen and others, in so far especially as they have aroused a personal interest in the problems of evolution, natural history instruction is given systematically in all schools and colleges, and the time seems to have come when, in the words of the naturalist first-named, "Scientific truth must cease to be the property of the few; it must be woven into the common life of the world." It is probable, indeed, that those who employ anatomical language to a greater or less extent at the present day are at least one hundred times as numerous as when Dr. Barclay's praiseworthy effort at reform was received with indifference or opposition.

It may be asked: In the face of this rapid popularization of anatomical knowledge is it worth while to introduce, or even to retain, any purely technical terms?

Apparently some German scientists have determined upon a negative reply to this inquiry, and their papers, even those of strictly scientific nature, teem with vernacular words, and with compounds thereof fearfully and wonderfully made.

If this kind of verbifaction be tolerable under any circumstances, it certainly would be justified by the extent and importance of the contributions to knowledge which appear first in the German scientific periodicals.

Upon this point, however, I can do no better than to quote the very recent judgment of one who is at the same time an investigator, a promoter of "the diffusion of knowledge," and an admirer of the methods and results of German science:

"Every art is full of conceptions which are peculiar to itself; and, as the use of language is to convey our conceptions to one another, language must supply signs for those conceptions. Either existing signs may be combined in loose and cumbrous paraphrases, or new signs, having a well-understood and definite signification, may be invented. Science is cosmopolitan, and the difficulties of the study of zoology would be prodigiously increased if zoologists of different nationalities used different technical terms for the same thing. They need a universal language; and it has been found convenient that the language shall be Latin in form, and Latin or Greek in derivation."—Huxley, C, 14.

Unless it can be shown that there is an essential distinction between the methods of designating entire organisms, and the parts thereof, the foregoing passages should silence the objections of those who would have us retain a vocabulary as vague as was that of chemistry in the days of lime, vitriol and copperas—a vocabulary which combines the ponderous stiffness of the cloister with the puerile vagueness of the nursery.

*Tuberculum bigeminum anterius* must give way to *lobi optici*, or some even shorter term; while *trachea* must take the place of *windpipe*, *weasand*, *luft-rohre* and *conduit arien*. Life is too short to spend in digging for truth with a long-handled shovel when a trowel will serve the purpose; nor is it becoming that any nation, however wise and great, should ask all the rest to take their intellectual food with chop-sticks of its peculiar pattern.

That there is no inherent obstacle to the employment of technical terms of classical derivation is shown by the readiness with which such words as *petroleum* and *phylloxera* have become domesticated along with the objects which they represent. There are scores of animals, like the *Rhinoceros*, *Hippopotamus*, and *Ichneumon*, for which there are no English vernacular names; while the youngest student of botany accepts *Hepatica*, *Anemone*, and even *Rhododendron* without difficulty or hesitation. Homely as it sounds, *stomach* is a strictly classical word, and the use of *caul* for *omentum*, or *sweetbread* for *pancreas*, would surprise a class in elementary physiology.

Even the late Jeffries Wyman, who saw no objection to *forearm*, and used *near* rather than *proximal* for the first row of *carpalia*, accepted *intermembral* as "good," and freely employed, if indeed he did not originate, the adjective *pretibial*, which probably would have come into general use had not the bone in question proved to be the homologue of the *intermedium*.—(Morse, 18, 13).

#### THE LIMITS OF TERMINOLOGICAL CHANGE.

As has been stated already, the modifications here proposed are intended to provide for what seem to be actual necessities, irrespective of purely theoretical considerations, and of any desire for a perfectly uniform and consistent terminology. It may be well, however, to specify certain general limitations to changes of anatomical nomenclature.

*Priority* is practically of little moment in respect to the names of organs, since it is usually difficult to ascertain when and by whom they were first applied. An example of this is afforded by the phrase *foramen of Monro*, (Wilder, 3). Nor, indeed, has priority always been held

sacred in systematic zoology. Owen's "Deinosaurians" was proposed nine years later than von Meyer's "Pachypoda;" yet, as stated by Huxley (108, 33), it has been retained, notwithstanding the small size of some members of the group.

*Etymological appropriateness* is sometimes disregarded, as in the case just mentioned, and in the more familiar names *Reptiles*, *Vertebrates*, *Edentates*, &c. Prof. Huxley has recently expressed the common sense view of the matter as follows:

"If well understood terms which have acquired a definite scientific connotation are to be changed whenever advancing knowledge renders them etymologically inappropriate, the nomenclature of taxonomy will before long become hopelessly burdened." (B, 751.)

So, too, the names of organs have sometimes been given in reference to some variable or unessential character, or have even represented an erroneous idea; yet no one now thinks of discarding either *rectum*, *arteria*, or *carotid*.

Sometimes even brevity and etymological accuracy yield to established usage. The word *cubitum*, proposed by me in 1872 (10, 21) as the technical equivalent of *forearm*, is both shorter than *antebrachium*, and more in accordance with its classical employment; but the latter word seems to be more generally preferred, and I am ready to accept it.

In another case, even though a new term has not yet come into general use, a special vitality may be imparted to it by the authority of those who may have adopted it. No marked or persistent disfavor is likely to be shown to terms which, like *myelon*, can claim Prof. Owen as father, and find a god-father in Prof. Huxley.

#### MESON, ITS DERIVATIVES AND CORRELATIVES.

The present tendency of accurate anatomical description is to refer the position or direction of all parts and organs to an imaginary plane dividing the body into approximately equal right and left halves; hence it is desirable to designate this middle plane, or any line contained therein, by a word which is at once significant, short, and capable of inflection. Dr. Barclay proposed *mesion*, and *mesial* has been generally used; but would it not be better to adopt the very term employed by the Greeks to signify the middle, *meson*, τὸ μέσον, equivalent to the more ponderous Latin *meditullium*? The corresponding adjective is *mesal*, and the adverb *mesad*, while in combination it becomes *meso*.

The following general terms were also proposed by Barclay, and have been more or less systematically employed by Owen, Huxley and others: *Dorsal*, *ventral*, *dextral*, *sinistral*, *lateral*, with the corresponding adverbial forms *dorsad*, etc. Should the alleged correspondence of the ventral region of the vertebrate with the tergal region of the arthropod prove to be one of true homology, it may be desirable in time to discard *dorsal* and *ventral* for more suitable terms, but for the present, if on practical grounds alone, it seems well to retain them.

#### CEPHALIC AND CAUDAL.

Barclay proposed *atlantal* and *sacral* for the designation of the position of parts lying toward the head or the tail in reference to an imaginary plane dividing the trunk at the middle of its length. But these terms were not applicable to parts beyond the atlas and the sacrum, so that new words were applied to the regions of the head. Perhaps this needless complication has hindered the general adoption of Barclay's nomenclature notwithstanding its many admirable features. At any rate, *cephalic* and *caudal* are much more acceptable terms, and are practically unobjectionable, although certain theoretical difficulties readily suggest themselves.

*Proximal* and *distal*, *central* and *peripheral* are in common use, and the general employment of their inflections and derivatives is only a question of time.

*Ental*, and *ectal* are here first proposed as substitutes for the more or less ambiguous words *inner* and *outer*, *interior* and *exterior*, *deep* and *superficial*, *profound* and *sublime*. Derived respectively from *ἐντός* and *ἐκτός* their significance is obvious, while their brevity and capacity for inflection will probably commend them to accurate working anatomists.

#### DESIGNATION OF THE REGIONS OF THE LIMBS.

Barclay's terms *ulnar*, *radial*, *tibial* and *fibular* refer to only two of the four aspects of each limb. Prof. Huxley has made the very important suggestion that, for comparison, all vertebrate limbs be regarded as placed in a *uniform normal position*; they are then extended laterad at right angles with the meson, with the convexities of the knee and elbow directed dorsad. Each limb then presents not only a proximal and a distal portion, but four general aspects, *dorsal*, *ventral*, *cephalic*, and *caudal*. Hence there appears to be no need for the introduction of the new terms employed to some extent by Huxley and other English anatomists, *epaxial*, *hypaxial*, *preaxial*, and *postaxial*. These words are also liable to misconception because *axial* has been used already in reference to not only the axis vertebra, but also the entire skeleton of the trunk as contradiistinguished from that of the limbs.

#### DESIGNATION OF CURVATURES.

Ordinary descriptions of the directions of curvatures are apt to be ambiguous, and Huxley resorts to the phrase "arcuated outwards" to indicate the form of the mandibular rami of the Balænoidea. Since the Latins designated the two malformations of the legs, "knock-knee" and "bow-legs," by the words *varus* and *valgus* respectively, we may find it convenient to speak of parts whose convexities look mesiad as *varate*, and of those whose convexities look laterad as *valgate*? In other cases, however, and perhaps even in these, so long as there is any opportunity for misapprehension, it will be well to describe curvatures as *presenting a convexity* in one or another direction. For instance, the mandibular rami of the Balænoidea present a *laterad convexity*, while those of the Physeteridae are convex toward the *meson*.

#### HYPOCAMPUS.

This is employed by Vicq D'Azyr in the descriptions of the plates of his *Traité D'Anatomie*, published in 1786. The more common form *hippocampus* occurs in the list of anatomical terms in the same volume, but this may have been compiled partly by others, while the descriptions are obviously the work of the anatomist himself. Vicq D'Azyr does not discuss the etymology of the term, but says the "*grande hypocrampe*" was first mentioned by Arantius and Varolius, whose works are not now accessible to me. Even Hyrtl does not seem aware of the use of the word by Vicq D'Azyr, and all other writers, so far as I know, make it *hippocampus*.

If the original orthography cannot be ascertained, *hypocampa* is to be preferred on etymological grounds; the ridges known as *hippocampus major* and *h. minor* bear no obvious resemblance to the fish known to the ancients as *ἵπποκάμπος* and *hippocampus*, but the larger of the two, which probably first received the name, does certainly present a most notable *downward curvature*, such as the Greeks might have designated by *ὑποκαμπή*.

#### DESIGNATION OF THE ENCEPHALIC CAVITIES.

As based upon the condition of things in man the current nomenclature of the ventricles had some slight foundation. But, in the light of better methods and more accurate knowledge, it appears incongruous and needlessly perplexing.

Let the learned anatomist lay aside his familiar acquaintance with the parts and their names, and put himself in the place of the beginner who, after gaining a general idea of the arrangement of the vertebrate brain from a

frog or menobranchus, is trying to master the complexities of the mammalian organ from the brain of the cat, dog or sheep.

Leaving the myelon, he finds the *canalis centralis* expanding into a cavity which, although the first of the series, is called the *fourth* ventricle. The more or less distinct cavities corresponding to the cerebellum and the *lobi optici* are not called ventricles at all, and the *third* is between the thalami. The two "lateral" ventricles are rarely mentioned as the *first* and *second*, but since the numbers must be understood in order to account for the *third* and *fourth*, the student desires, in vain, to know which is the first and which the second. In point of fact, if the enumeration is begun at the cephalic end of the series, the lateral ventricles are the third and fourth, since there are well-developed ventricles in the *lobi olfactorii*. Finally, a "*fifth* ventricle" is mentioned, which is not only at the greatest distance from the fourth, but has no normal connection with the other ventricles, and is, in fact, no part of the series.

In view of all this, the task of describing to students the highways and by-ways of the brain,—which should be most attractive because therein is most clearly manifested the ideal arrangement of the organ,—is one from which I shrink as from any other kind of solemn nonsense. To my mind, indeed, rather than go on as we have been going, it would be at once more philosophical and more intelligible to adopt the simple vocal device employed by Straus-Durckheim for the designation of the metatarsalia—"padion, pedion, pidion, podion, pudion"—and to re-christen the ventricles by, for instance, the names *pran*, *pren*, *prin*, *pron*, and *prun*.

Fortunately, however, another alternative is presented. Whatever objections may be urged against them on theoretical grounds, a real practical advantage is gained by the use of the terms *rhinencephalon*, *prosencephalon*, *diencephalon*, *mesencephalon*, *epencephalon*, and *metencephalon*, and their German or English equivalents are likewise often employed for the designation of the general regions of the brain. Assuming that these terms are to be retained, and that they are to be learned by successive generations of students, why should we not transfer the distinctive prefixes to the Greek word for ventricle, *cœlia*, *κοιλία*? This would give us *rhinocœlia*, *procœlia*, *dicœlia*, *mesocœlia*, *epicœlia*, and *metacœlia*.

These terms are capable of inflection, and the longest of them is no longer than the Latin *ventriculus*, which requires a prefix or qualifying word. Lastly, but by no means of least importance, they correspond with the names of the encephalic segments. As will be seen in the list of names of the parts of the brain, these prefixes are employed for the designation of the membranous roofs of the "third" and "fourth" ventricles, and the plexuses of these and the lateral ventricles. After a somewhat prolonged consideration of the matter, it seems to me that the practical usefulness and logical consistency of these new terms outweigh any objections that may be urged, and that these latter are less numerous and serious than could be brought against any other substitutes for the present heterogeneous and ill-applied nomenclature.

Two or more ventricles may be spoken of as *cœlia*, while the "fifth" may be called *pseudo cœlia*. I hope, before long, to justify more fully the proposition already made\* to consider the cephalic portion of the "third" between the *portæ* (foramina Monroi), as a morphologically independent cavity under the name of *aula*.

#### RHINEN, ETC.

May not *rhinen*., *prosen*., *dien*., *mesen*. and *epen*. be written, for the sake of brevity, for the full titles of the general divisions of the brain, *rhinencephalon*, *prosencephalon*, etc?

\* Proceedings of the Am. Assoc. for Adv. of Science, Aug. 25, 1880 reported in "New York Medical Record."



The following abbreviations are printed in Webster's Dictionary without the period: *etym(on)*, *demirep(uta-tion)*, *grog(ram)*, *hyp*, and *hypo(chondria)*, *noncon-(tent)*, *hyper(critic)*, *navvy* for *navigator*; but the abbreviations above suggested should probably be followed by the period.

#### PRÆCOMMISSURA, ETC.

The single words *præcommissura*, *medicommisura*, and *postcommissura* are proposed as substitutes for the compound terms *commissura anterior*, *medius*, and *posterior*, and for their English equivalents. A similar change is desirable in the case of the three cerebellar peduncles, which may be more conveniently termed *præ-meso-* and *postpedunculus*. So, too, the *corpora geniculata* (external and internal) may be called *prægeniculatum* and *postgeniculatum*; the *brachia* of the mesencephalon become *præbrachium* and *postbrachium*, and the two "perforated spaces," *præperforatus* and *postperforatus*. The "anterior pyramids" have been called by Owen "prepyramids," but more exact designations of these and of the "posterior pyramids" would be *ventri-pyramides* and *dorsipyramides*.

The prefixes are usually employed when the object referred to lies before, between, or behind other objects of a different kind; *e. g.* *præcordia*, *mediterraneus*, and *posterganeus*. The use here proposed is as if three dogs in line were designated by *præcanis*, *medicanis* and *postcanis*. If the terms are objectionable, what can be substituted for them? They are certainly as legitimate as are the well-established terms *prosencephalon*, *mesencephalon* and *metencephalon*. Do not the English words *preposition* and *postposition* offer some analogy?

The following points are mainly etymological and orthographical rather than anatomical.

#### THE CONNECTING VOWEL.

With derivative words the connecting vowel is commonly *i*; *e. g.* *alipes*, *claviger*, *fatifer*, *fidicen*, *fluctigena*, *decimanus*, *neurilemma*, and *xiphisternum*. But classical exceptions are *mulomedicus*, *quadrupedus*, *noctuvigilus*, and *decumanus*. In common English and scientific terms of Latin or Greek origin the *o* is common; *e. g.* *ambodexter*, *burgomaster*, *gastrotomy*, *termonology*, *ventroinguinal*, *lateroflexion*, *mucopurulent*, *vasomotor*, *curvograph*, *neuroglia*, *oculospinal*, *pleuroperitoneal*, *xiphosura*, *septopyra*, *hemoglobin*, *cephalotribe*, etc. Rarely is it *e* as in *venesection*.

Should the *i* or the *o* be used in the following terms: *Dorsimeson*, *ventrimeson*, *dorsicumbent*, *latericumbent*, *dextriflexion*, *sinistriversion*, *cephaloduction*, *caudiduction*, etc.? Both analogy and euphony lead one to use the *i* when the first part of the word is of Latin origin, and the *o* with the Greek.

Should any of these terms be written as compound words?

#### COMPOUND WORDS.

The two Latin compounds known to me are *veneri-vagus* and *vesti-contubernium*. The following common or technical English compound words are selected from Webster's English Dictionary, or the Medical Dictionaries of Dunglison, or Littré et Robin, or from the writings of Barclay, Humphrey, and Straus-Durckheim: *Anglo-Saxon*, *concavo-convex*, *dextro-gyrate*, *ventro-appendicular*, *costo-vertebral*, *costo-alaris*, *caudo-pedal*, *osseo-cutaneous*, *occipito-scapularis*, *dorso-lateral*, *sterno-clavicular*, *clavo-cucullaire*, *clavi-sternal*, *clavio-humeralis*. By analogy with the foregoing, compound terms of direction should read *dorso-ventral*, *caudo-cephalic*, *meso-lateral*, *sinistro-cephalic*, etc.

#### HYBRID WORDS.

Some of the terms already mentioned are formed by the union of Latin with Greek words; *e. g.*, *dorsimeson*,

*meso-lateral*, and *caudo-cephalic*; several others are likely to be employed; *e. g.* *clavo-mastoidæus*, and *felistomy*.

Beyond the occasional intimation, in the dictionaries, that a term is hybrid, the subject seems to be ignored, and it might fairly be inferred that literary authorities entertain one or the other of two opposite convictions: either mongrel words are verbal monstrosities which will be shunned instinctively by all well-regulated minds, or there is no more serious objection to their use, or even their creation, than to the employment, or even the production, of mules, or the mixed varieties of grapes and roses.

However this may be, the fact is that the Latin and the Greek tongues have united to form the following nine hybrids which may be found in Latin writings: *anticato*, *biclinium*, *cryptoporticus*, *dentarpage*, *epitogium*, *monosolis*, *monoloris*, *pseudo-flavus*, and *pseudo-urbanus*. Of these, the third only occurs with any degree of frequency.

Whoever will spend the time to look through an unabridged dictionary of the English language—and the interest as well as the instructiveness of such a search can hardly be realized by those who use the volume only for occasional reference—will find that, after excluding the twenty-five or more words ending with *meter*, which may perhaps be derived directly from the Latin form *metrum*, there are more than one hundred hybrid words, many of them in good standing. Many more are to be gleaned from the dictionaries of medicine and the other arts and sciences.

Nevertheless, it is probable that a due regard for the feelings of the classical purists in whose eyes language was not made for man, but rather man for language, will lead scientists to refrain from the introduction of mongrel terms when others will serve the purpose, and the present writer will be pleased to receive suggestions leading to the substitution of wholly unobjectionable words for any of the hybrids which have been mentioned.

(To be continued in our next.)

### ON CHICKEN CHOLERA: STUDY OF THE CONDITIONS OF NON-RECIDIVATION AND OF SOME OTHER CHARACTERISTICS OF THIS DISEASE.\*

BY M. L. PASTEUR.

#### II.

Concerning the properties of the extracts of the artificial cultivation of the germ of chicken cholera, an inquiry presents itself. We have shown that these extracts contain no substances capable of preventing the cultivation of the germs of this disease. They might, however, contain elements adapted to the vaccination of chickens. To investigate this point I have prepared cultivations where volume was not less than 120 c.c. After filtration and evaporation at a low temperature, while infinite care has been taken that its purity should not be affected, this liquid has given a dry extract, which was re-dissolved in 2 c.c. of water, and the totality of this was injected under the skin of a chicken which had never had chicken cholera. A few days afterwards the chicken, after being inoculated with a virus of the greatest virulence, died with the usual symptoms of *unvaccinated* chickens.

This class of experiments led to the following observation, which is of the greatest importance in physiology. When the extract from the cultivation of the germ of this disease, corresponding to an abundant development of the parasite, is injected under the skin of a fresh chicken in perfect health, the following phenomena take place: At first the chicken seems to suffer from a nervous dis-

\*Translated from the *Comptes Rendus de l'Académie des Sciences*, of May 3d, 1880, by P. Casamajor. The translation of the second paper of this series appeared in the *Chemical News*, vol. xlii., page 321 (December 31, 1880).



order, which is indicated by panting breath and alternately opening and closing its beak; afterwards it becomes motionless, assumes the shape of a ball, refuses food, and seems overcome with drowsiness, as is the case with chickens affected with the disease. There is this difference, however, that the chicken wakes up at the least noise. This sleep lasts about four hours, after which the chicken wakes up, looks as well as usual, eats and cackles as if nothing had happened to it.

I have repeated this experiment several times and have always observed the same facts. Before injecting the extract above mentioned, I took, in every case, the precaution of injecting an extract of the pure chicken broth, which does not cause analogous phenomena. I have, by this means, acquired the conviction that, during the life of the parasite, a narcotic is formed, and that it is this narcotic which causes the morbid symptom of sleep so characteristic of the disease which we are studying.

By the acts of its nutrition the germ of the disease causes grave disorders and brings on death. The germ, being aerobian, absorbs during its life large quantities of oxygen, and burns up many of the elements of its medium of cultivation. This may be seen by comparing the extract of the broth, before the development of the germ, with the extract of the liquid in which the development has taken place. Everything seems to show that it is from the globules of the blood that the oxygen necessary to its existence is derived by absorption through the tissues. While the chickens are still alive, even when death is still far off, their combs assume a violet tinge at a time when the germ of disease is so little diffused through the blood that it escapes microscopical examination. This species of asphyxia is one of the most curious traits of the disease we are studying. Death is caused by the grave disorders brought about by the development of the parasite in its body, by pericarditis, by serous extravasations, by alterations of its internal organs, by asphyxia, but the sleep characteristic of the disease is caused by a product formed during the life of the germ, which acts on the nervous centres. The independence of these two effects in the symptoms of this disease is further established by the fact that the extract from a filtered cultivation of the germ acts as a narcotic on chickens which have been submitted to the *maximum* degree of vaccination.\*

These facts will, doubtless, be found worthy of the meditations of pathologists.

Although I have taken already much of its time with this subject, the Academy will allow me to call its attention to some other characteristics of the disease called *chicken cholera*. We know that this disease is rapidly fatal, particularly if caused by a direct inoculation of its germ. It must then appear extraordinary that it sometimes presents itself in the chronic state, as in the case of inoculated chickens; which, after being severely ill, do not die, but seem to get relatively better. They eat, however, very little; they become anæmic, as shown in the discoloration of their combs; they continue to lose flesh, and finally die, after lingering for weeks or months. This fact would not be of primary importance if, at the death of the chicken, the germ of the disease was not, in most cases, found in its body, which conclusively proves that the parasite has been present since the last inoculation, always active, although in a mild form, for it brings on death slowly. Doubtless, the germ was placed in some *vaccinated* portion unfavorable to its cultivation. Vaccinated chickens are most apt to present this form of disease, which is of very rare occurrence. We might be led to believe that, in this case, the virulent virus is changed into the attenuated, but this would be an error. In cases of this kind the virulence of the germ of the dis-

ease seems, on the contrary, to be aggravated. This may be easily seen by cultivating it artificially, so as to separate it from the blood, and inoculating it on fresh chickens.

Facts of this kind help us to understand the possibility of those long incubations of virus, such as that of rabies, for instance, which, after existing a long time in the body in a state which may be called latent, suddenly manifest their presence by the most marked virulence and by death. Do not these facts also throw light on human pathology?

Alas! how often we see virulent diseases, such as scarlatina, measles, typhoid fever, followed by serious disorders of long duration, which are frequently incurable? The facts to which I have called attention are of the same nature, only here we can put our finger on their true cause.

I will conclude by pointing out another peculiarity, which is not less worthy of the attention of the medical profession.

In chickens in perfect health which have been thoroughly vaccinated there often occurs an abscess full of pus on some portion of the body, which does not seem to have any injurious effect on the health of the animal. It is a remarkable circumstance that this abscess is due to the germ of chicken cholera, which remains in it as in a closed vessel, and it cannot propagate, doubtless, because the chicken has been vaccinated. This germ may be withdrawn by artificial cultivation, or it may be directly inoculated on fresh chickens, which it kills in the usual manner after an abundant development. These facts recall the abscesses on guinea-pigs, which I have mentioned in the first communication on this subject, and they furnish a rational explanation of what happens in these abscesses. In all likelihood the muscles of the guinea-pig cultivate the germ more slowly and with greater difficulty than those of chickens; the disease is limited to an abscess, and recovery becomes possible.

I will now conclude this statement, as I have no wish to wear out the patience of this Academy. This subject is, however, so vast and so fruitful that I will ask its permission to bring the subject before it again. I have other observations to present than these. I will add those which will present themselves in the investigations I am now making.

"We would give nothing to the public," said Lavoisier, "if we waited until we reach the end of our researches, as these become broader and more extended the farther we advance."

#### THE NEW CHEMISTRY.

Lieut.-Col. W. A. Ross, who has done so much to advance our knowledge of blowpipe analysis, and whose original chemical investigations are of the greatest interest, in speaking of Prof. Cooke's "New Chemistry," indicates as follows, that much more radical changes, at all events as regards anhydrides, will shortly demand the attention of philosophical chemists, in consequence of the following facts:

FACT 1. 5mgrs. of pure caustic lime are carefully fused into a bead of pure boric acid before the blowpipe, the bead boiled in distilled water, and the transparent calcium borate ball thus extracted, weighed. The weight will invariably be exactly 20mgrs.

FACT 2.—The above-mentioned ball is now fused into a second bead of boric acid, the transparency of which it does not in the least degree affect, and when again boiled out it has the same weight—viz., 20mgrs.

FACT 3.—5mgrs. of calcium hydrate are now fused into a boric acid bead similarly to (1), when it is observed that the borate ball formed is at first opaque white; then, as it becomes transparent B B, that an enormous amount of opalescent matter is emitted from the ball into the bead; and finally, that the extracted ball weighs only 15mgrs.

\* I should, however, try to isolate the narcotic, and see whether a sufficient quantity could cause death, and whether, in this case, the internal disorders would be the same as those of the disease itself.

FACT 4.—The calcium borate ball (2) is now held as a bead *per se* on platinum wire, and 2.5mgrs. of pure silica, or of rock crystal, dried at red heat, dissolved in it B B: after which the silicious ball is weighed, and added B B to a boric acid bead, which it NOW *renders opaque with opalescent matter*; finally, the extracted ball, when weighed, showed, in an average of three assays, an increase in weight of 42 per cent.

FACT 5.—5mgrs. of pure “anhydrous” silica ( $\text{SiO}_2$ ) are carefully taken up on a bead of pure boric acid, and observed to be absolutely unalterable there, B B. A weighed ball of anhydrous calcium-borate is now added B B to this bead, when the silica is gradually decomposed—the weight of the ball being unaltered—not into silicon and oxygen, but into really anhydrous silica (*which possesses extraordinary electrical properties*), and some compound of hydrogen, which makes the bead opalescent. After boiling, only 2mgrs. of residue are obtained.

Now these five facts, and more especially the immense increase in weight of the silicious calcium-borate ball (4), notwithstanding the great loss of matter causing opalescence, show that there is an enormous percentage, nearly half, of SOME COMPOUND OF HYDROGEN, not eliminable as gas, existing in what has been hitherto supposed to be an anhydrous substance, which has escaped even the closeness of modern chemical analysis, for the simple reason that the water solutions of acids and alkalies used to analyse, themselves contain this very compound of hydrogen.

Many confirmatory proofs of this startling truth have been afforded, but cannot be detailed here, because the details form part of the subject of a competency essay, and cannot yet be published.

Hydrogen, however, in this solid form, can now be proved to be an almost omnipresent component—of all so-called “anhydrous” minerals, of most artificial as well as natural inorganic productions, of many so-called “elements,” and, to my mind, of the galvanic “currents” themselves.

Thus it is seen that the beautiful and immaculate theory of combining proportions, first enunciated in 1777 by the illustrious Wenzel in his “*Lehre Von der Verwandtschaft der Körper*,” relates entirely to hydrates, and that a new chemistry, the chemistry of anhydrides, now requires to be studied.

Let us hope that some future Wenzel and Dalton will apply proportional and atomic theories to this anhydride chemistry, and now that the first dawning of the truth has at last been published in Germany and America as well as here, we cannot doubt that this will soon be done.

It remains, now, an unpleasant part of my duty to point out that, although I supposed, by the discovery of the above mentioned facts, I had laid the first foundation of what must, sooner or later, be adopted as a new and essential study by everyone who aspires to the title of a philosophical chemist, I found I had been anticipated in my most important deductions by no less a man than Joseph Priestley.

That unfortunate genius—in repeating one of whose experiments with a more powerful electric battery, Sir Humphry Davy discovered potassium—has been so utterly misrepresented by the modern school of chemists, which has elevated Lavoisier in his place as the founder of the chemistry of hydrates, that it would take more time than you and I can afford, to adduce in proof, a quarter of their misrepresentations.

I will give just one instance. Prof. Cooke, in the book called “*The New Chemistry*,” says (p. 98): “Iron, in rusting, gains in weight, ‘Hence,’ said Lavoisier, ‘it has combined with some material.’ ‘No,’ said such men as Cavendish, Priestley, and Scheele, ‘it has only lost phlogiston, which differs from your gross forms of matter in that it is specifically light, and, when taken from a body, increases its weight.’ We smile at this idea,” etc.

Now what does Priestley himself say?—See p. 249,

Vol. I., “*Experiments and Observations*,” sect. IV., “*In flammable Air*.”—“It was even asserted by some that phlogiston was so far from adding to the weight of bodies, that the addition of it made them really lighter than they were before, on which account they chose to call it the principle of levity!” Priestley says here, that he “discovered phlogiston to be hydrogen by direct experiments.”

Then follow those celebrated experiments—so much neglected and concealed by modern chemists—in which Priestley converted a certain quantity of lead oxide into a certain quantity of lead “by throwing the focus of a burning-lens upon it through a glass receiver filled with a certain quantity of “*inflammable air*”—or hydrogen.

It may be fashionable now to “smile at the ideas of such men as Cavendish, Priestley and Scheele”; but it seems to me much more reasonable to smile at the ideas of Lavoisier and his disciples, who did not seem able to understand the possibility of a compound losing (by means of heat or other factor in the operation) an extremely light constituent, and taking up, instead of it, another surrounding constituent sixteen times as heavy, whereby the aggregate weight of the compound would, of course, be increased by the coefficient fifteen.

In precisely the same way I have proved, by my humble experiments, that a ball of calcium-borate, having silica (for instance) dissolved in it, increases enormously in weight by treatment in boric acid B B, although it obviously loses a large quantity of hydrogenous matter, which renders the whole bead opaque white; simply because the compound acquires, instead, a much heavier constituent—viz., boric acid.

We invite those who are interested in the blow-pipe analysis who desire any information on the subject to address a letter to “*SCIENCE*,” as Col. Ross is one of our subscribers, and appears always ready to aid those who require instruction. A letter to “*SCIENCE*” will doubtless receive prompt attention.

#### THE AMERICAN CHEMICAL SOCIETY.

The March meeting of the American Chemical Society was held on Monday evening, the 7th inst., Vice-president Squibb in the chair. The resignations of the following gentlemen were read and accepted. Messrs. Elihu Root, H. G. Smith, F. Alexander, J. T. O'Connor, and also, in consequence of its interference with his business, the Recording Secretary Dr. A. H. Gallatin, tendered his resignation from office. Mr. Theodore Tonnelé and Mr. J. G. Mattison were nominated for membership. The reading of papers followed, the first of which “*A New Specific Gravity Bottle*” by William H. Gregg, was read by Dr. A. Behr. The essential difference between the ordinary bottle and the one devised by Mr. Gregg consists in that the latter has an expansion or bulb just above where the stopper is, in the regular form. A thermometer serves as a stopper passing through the bulb sealing it at both extremities. The advantage of this improvement is that the liquid cannot run over or volatilize (in the case of essential oils, etc.) for it will be retained in the bulb which is stoppered at each end by the thermometer.

The second paper was by Dr. J. H. Tucker, “*On the solvent action of carbonic anhydride in solution upon various bodies under different conditions as to temperature and pressure*.” The methods of manipulation were first detailed, after which the effect upon the “various bodies,” these being chiefly mineral, was described. Mr. Casamajor followed with some observations upon the difficulty that he had experienced in obtaining hydrogen sulphide from impure iron sulphide. After some experimenting he found that upon adding a little zinc amalgam, alopian evolution of the gas ensued. By this method he was successful in obtaining excellent results with galena, orilcopyrite and pyrite.

Mr. J. H. Stebbins, Jr., called the attention of the society to several new coloring materials that he had discovered among the di-amido compounds. They were yellow in color and suitable for silk, woolen and cotton dyeing, but especially desirable for the latter.

Dr. A. R. Leeds gave a short description of some new experiments on the action of hydrogen peroxide with ammonium hydrate.

A committee consisting of Mr. Casamajor and Dr. Alsberg were appointed to make arrangements for the annual dinner.

M. B.

NEW YORK, March 9, 1881.

## IMPROVED PORTABLE EQUATORIAL STANDS.

By JAMES H. GARDINER.

The stand I use, and those which I have seen, have no levels and no means by which the telescope can be moved in azimuth without moving the whole stand. It seems to me that, with a very little trouble, these stands could be made not only a great deal more accurate, but also much more useful for amateur work by the following additions: Instead of having the equatorial mounting screwed firmly to the lower plate to which the legs are attached so the telescope cannot be moved in azimuth without moving the whole stand, a plate could be ground to touch, say, only  $\frac{1}{4}$  of an inch, and revolved on the lower plate. This would give a steadier and easier motion, with less friction than if the two plates were ground to touch all over. A thread is to be cut in the side of this upper plate, so that with a tangent screw it can be moved in azimuth. On this upper plate that revolves on the lower plate, and to which the tangent screw is attached, should be placed two levels at right angles to each other, and then on this upper plate that revolves the usual equatorial mounting is to be firmly fastened. It will be seen that the above stand only differs from the usual stands on tripods, in having levels and means to move the telescope in azimuth without moving the whole stand. Such a stand would be of great use to amateurs, who have a poor horizon, and are obliged to move their stands about to command all parts of the heavens; or for those who may have a good horizon, but cannot afford the luxury of a fixed pillar and dome. The use of such a stand will appear from the following illustration; Suppose the observer has such a stand, and that he is at Washington, and on the 1st of March, 8 P. M., he desires to put his telescope in the meridian. He carefully levels the stand, and turns his telescope on a *Polaris* to come into the centre of the field. If it does not happen to come exactly in the centre of the field, he can raise or lower his polar axis, or move the telescope in azimuth by aid of the tangent screw. Here it is to be noted that with the old stands he would have to twist the whole stand around and throw it out of level, and by repeated trials get a *Polaris* in the centre of the field, and when he again levelled the stand a *Polaris* might not be in the centre of the field. Thus every movement of the old stand would throw it out of level. All these tedious trials are obviated by the new stand with azimuth motion. When once levelled it would stay so, and the telescope could be moved to the east or west without having to be continually bothered with levelling it. Thus in a few moments he would have a *Polaris* in the centre of the field, and the telescope approximately in the meridian. He now reads his R. A. circle, and turns his telescope on some well known star, as a *Leonis* or *Regulus*, for example, and then reads his R. A. circle again. Supposing the difference of these two readings of the R. A. circle to be 3h. 25m. 13s., this is the observed hour-angle of *Regulus*. The true hour-angle of *Regulus* is equal to the difference of the Sidereal time and the R. A. of *Regulus*, or 3h. 22m. 13s. This shows that the object-end of the telescope must be moved 3m. to the west to make the observed hour-angle agree with the true

hour-angle. This can be done nicely by the tangent screw that moves the telescope in azimuth without throwing it out of level, but with the old kind of stand it would be thrown out of level, and it would be a very tedious job, requiring time and patience to accomplish. Having got the telescope very nearly in the meridian, the declination circle can now be set to the  $\delta$  of the star. With such a stand the careful amateur can put it near enough in the meridian to pick up a comet or any other object by its R. A. and  $\delta$ . The accuracy of the adjustments depends upon the levelling, the collimation, and an exact value of the local time. The levelling would generally be accurate enough, and most stands have screws in the saddle that carries the telescope for correcting the collimation. But the amateur should try to get the exact value of his local time, as this would probably introduce the greatest error. This can be done by equal altitudes of the sun or star. Or where the latitude of the place is well known the local time may be found by an altitude of the sun. With such a stand as has been described, if it should be necessary to move it to another place, it could easily be put in the meridian again, as described. Besides, many have stands with good circles which they seldom use, because they cannot afford a fixed pillar and dome, and do not care to put it in the meridian, as they are obliged each night to bring the telescope into the house. But if it could be put in the meridian easily, I am sure many would be pleased to use their circles.

## ASTRONOMICAL MEMORANDA.

[Approximately computed for Washington, D. C., Monday, March 21, 1881.]

Sidereal time of mean noon, 23<sup>h</sup>, 57<sup>m</sup>, 24<sup>s</sup>. Equation of time, 7<sup>m</sup>, 8<sup>s</sup>. Mean noon *preceding* apparent noon.

On the morning of March 20th, the sun crosses the equator and enters the constellation Aries, thus indicating the commencement of Spring. The violent actions upon the sun's surface have continued throughout the past month.

The *moon* reaches its last quarter on March 22, and is new again on the 29th. On March 21st, she crosses the meridian at 4 A. M. The moon will be in conjunction with Mercury on the 27th, and with Jupiter and Saturn on the morning of the 31st.

*Mercury* is morning star, crossing the meridian about an hour before the sun, nearly 6 degrees farther south. Mercury was in inferior conjunction with the sun on the 11th and is travelling towards the west.

*Venus* has been moving westward since her greatest eastern elongation on the 20th of February, and will continue to increase in brilliancy till March 27th. She crosses the meridian at about 2.40 P. M., about 20 degrees farther north than the sun.

*Mars*, crossing the meridian nearly 3 hours in advance of the sun, is coming towards us, and gradually increasing in brilliancy.

*Jupiter* crosses the meridian at about 1.15 P. M., and *Saturn* 15 minutes later. They are both becoming very unfavorably situated for observation, and must be looked for immediately after sun-set.

*Uranus* is in right ascension 10<sup>h</sup>, 50<sup>m</sup>, 47<sup>s</sup>; declination 8° 14' north, and was in opposition on March 1st.

*Neptune*, right ascension 2<sup>h</sup>, 47<sup>m</sup>, 17<sup>s</sup>; declination 13° 56' north. Neptune and Venus are in conjunction on the 23rd.

THE following is a list of the officers and council of the Royal Astronomical Society, elected February 11, 1881:—President: J. R. Hind; Vice-Presidents: Prof. Cayley, E. Dunkin, W. Huggins, E. J. Stone; Treasurer: F. Barrow; Secretaries: W. H. M. Christie, J. W. Glaisher; Foreign Secretary: the Earl of Crawford; Council: Prof. Adams,

Sir G. B. Airy, J. Campbell, A. A. Common, G. H. Darwin, Major J. Herschel, E. B. Knobel, G. Knott, A. Marth, E. Neison, A. C. Ranyard, Prof. H. J. S. Smith.

THE gold medal of the Royal Astronomical Society has been presented to Prof. Axel Möller, Director of the Observatory at Lund, in Sweden, for his investigations on the motion of Faye's comet. W. C. W.

WASHINGTON, March 18, 1881.

### MICROSCOPY.

On looking over the Transactions of the New Zealand Institute for 1878, we notice that a Mr. A. Hamilton speaks of having discovered *Melicerta ringens*. It was found in great profusion, on the finely-divided leaves of the *Myriophyllum*. This adds another locality to the wide geographical distribution of this interesting Rotifer.

Mr. Hamilton states that after examining a number of specimens he found the description given by Gosse correct, except that the formation of the pellets was at a much slower rate than that stated by him.

In the same locality were also found organisms which Mr. Hamilton thought to be *Plumatella repens*; they were growing on dead thistles in a swamp in only a few inches of water.

*The American Monthly Microscopical Journal* for March editorially announces the immediate publication of Mr. F. Habirshaw's Catalogue of the Diatomaceæ, also by the editor, a small book based on Professor J. Leidy's "*Freshwater Rhizopods of North America*." The editor's handbook on Adulteration is withdrawn.

In the same number Dr. F. S. Billings gives a long resumé of what is known about "*Trichina*," but seems to offer no new facts; the illustration he offers of "*Fresh trichinous invasion*" (after Heller) is a wretched misrepresentation of free trichinæ.

Any reader desirous of examining living specimens of trichinæ in this condition can obtain them on calling at our office.

### LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

#### To the Editor of "SCIENCE:"

The development of a peculiar non-nervous tissue in connection with the rhomboid sinus of the lumbo-sacral intumescence in birds, and which is especially well marked at the embryonic period, is I think of some bearing on the recently agitated question of a so-called lumbar brain in the extinct sauranodon. In all amniote embryos that I have studied myself, and of which I can find illustrations in accessible works, it is remarkable that there is a distinct posterior enlargement before the cephalic enlargement is well marked, or the brachial intumescence is even indicated in the medullary tube. This fact may point to the potent influence of some, at one time, deeply engrafted ancestral trait. It is not, I think, necessary or warrantable to go beyond this fact, and the established one of the existence of a non-nervous enlargement at the same region in allied sauropsidæ in endeavoring to account for the peculiarity found in the spinal canal of an extinct saurian genus. The supposition of the existence of anything meriting the designation of a brain elsewhere than in the cranial cavity in any amniote animal would be so fundamentally out of harmony with what we have learned to consider as the normal type of structure, that much stronger evidence than the size of a bony receptacle must be adduced be-

fore it can even be taken into consideration. That the size of a cavity and that of the contained organ are not necessarily in close correspondence, has been alluded to by another correspondent under the initials of B. G. W. I have been struck, in this connection, with the discrepancy between the size of the brain cavity and the brain itself in a two year old hippopotamus, though they corresponded in a young elephant.

Respectfully,

E. C. SPITZKA, M. D.

N. Y. 130 E. 50th Street.

#### To the Editor of SCIENCE:

PARIS, March 5th, 1881.

In bringing before your notice various points which are both novel and interesting, it seems to be my fate constantly to struggle with an *embarras de richesses* represented by a vast combination of phenomena which is forever appearing upon the scientific horizon.

Condensing therefore as much as possible the matter at my command, I will begin with a very trite and commonplace observation; petroleum is a most excellent thing in its way. It is inexpensive and it gives forth a beautiful light. But these advantages, as many know to their sorrow, are more than counterbalanced by the disagreeable habit it sometimes has of exploding. Accidents thus occasioned, frequently prove fatal, as the violence and intensity of the explosion prevent, in most cases, speedy relief being administered to the victims. Besides this, the methods employed are inefficient and usually unsatisfactory.

M. Ichlumberger, whose mind for some time has been occupied with this subject, finally proposes a mode of extinction which is exceedingly simple, and at the same time instantaneous. So confident is he of the efficacy of his plan, that he would like to make a law compelling every one to adopt it who has petroleum in any quantity.

This is his method; Upon every keg or barrel of petroleum, place a moderately large bottle filled with aqua ammoniæ. Should an explosion occur, the shock will shatter the bottle, spread the fumes of the ammonia in the atmosphere, and produce an automatic and infallible extinction of the flames.

This plan can well be recommended to those who make use of petroleum, or who are obliged to superintend the distillation of the liquid. It is only necessary to have within easy access one or several bottles of aqua ammoniæ, whose contents should instantly be scattered upon the petroleum in case it catches fire.

M. Ichlumberger also thinks that this mode of extinction could be effectively utilized in mines where fire-damp is imminent. The ammonia should be put in reservoirs, and so placed that it will be overturned immediately when the explosion occurs. This agent would undoubtedly be more powerful than water, and M. Ichlumberger's idea is worthy of serious attention.

A very peculiar case of poisoning occurred a short time ago at *Puy l'Evêque*, an account of which was sent to the *Académie de Médecine* by Dr. Demeaux. It seems that a family composed of five persons was taken violently ill after having eaten some mushrooms. One of the mushrooms left from dinner was sent by Dr. Demeaux to the *Académie* as a specimen, and upon being examined by M. Chatin, was found to belong to one of the numerous varieties of the *orange-ciguë* species called the *Amanita bulbosa*. Nine-tenths of the mushroom poisoning we hear about is due to this *Amanita* which, on account of its white color is frequently mistaken by the inexperienced and unsuspecting for the harmless mushroom. It is certainly the height of folly for people to run about the woods and fields mushroom hunting, unless they are perfectly familiar with the different species.

Science, it seems, is able to reap some benefit from everything, however trivial! Fancy the ignoble art of Tattooing being elevated to a philanthropic institution! And yet, this is indeed the case. Up to the present day, the artists whose business it was thus to decorate the human skin, confined themselves to tracing merely a warlike emblem in indigo or vermillion upon the arms of our troopers, with the number of the regiment added sometimes. However, their ambition led them to execute a more intricate and ornamental design, such as a flaming heart pierced by an arrow, accompanied by the inscription, "To Mary," or something equally effective. Henceforth, be it understood, these dermographic artists will be looked upon as valuable auxiliaries to surgery.

"Why is it," asks Dr. le Comte, who is physician to a regiment of dragoons, "why is it that such quantities of soldiers die upon the battle field?" And then he replies confidently: "Simply because of the difficulty which arises in regard to arresting hemorrhages."

The compression of an artery being the best mode of stopping profuse bleeding, Dr. le Comte proposes to teach each soldier first where these vessels are situated, so that he may assist himself while waiting for the surgeon. Therefore, he tattoos an image of some kind upon every portion of the soldier's body where there is an artery. Think of it! Has ever a more ridiculous and absurd idea been put into practice? How infinitely preferable it would be to furnish each soldier with a tourniquet, or at least compel him to attend six lectures upon anatomy, even though such a course might spoil a good soldier to make a bad doctor.

I believe some news has already reached you of Balmain's luminous painting, which attracted public attention some months ago and was first practically applied at the establishment of Messrs. Thlee and Horm. The ceilings of their different offices were covered with a layer of the composition, dissolved in water, and the effect produced is that of a diffused light which is sufficient to enable one to distinguish the various objects in the room.

M. Balmain's idea is excellent, and it would be most advantageous to paint the ceilings of rooms, passages, halls, etc., with his composition, should the use of lamps be dangerous or not absolutely necessary. A simple border of the painting is sufficient in narrow passage ways and stair-cases, and costs a mere nothing.

When dissolved in water, the composition can be applied like whitewash or kalsomine and is useful in more ways than one. Large slabs of glass have been covered with it and employed on board of English marine vessels, also in the Waltham powder factory and in Mr. Young's refinery to illumine places where it is impossible to carry a light. This painting has likewise taken the place of lamps upon several railroads in England, particularly those lines where tunnels are so frequent as to necessitate constant light in the carriages.

Now, a word about meteorology. Nearly every book that has been written on the subject, tells us unhesitatingly that the *Aurora Borealis* is a very rare occurrence except in the polar regions. It appears, however, that this is by no means the case, and that it can be observed with equal frequency in countries occupying a much lower latitude.

M. Sophus Tromholt, of Bergen, Norway, has just published an interesting account of some observations made at his request during the winter of 1878-1879, at one hundred and thirty-two stations extending throughout Sweden, Norway and Denmark. Many extraordinary facts concerning the *Aurora Borealis* can be gathered from this work. To give you an example, it was found that scarcely an evening passed that the phenomenon was not witnessed in one of these countries. M. Tromholt thinks the Aurora is often a local phenomenon, situated but a short distance above the surface of the earth. To strengthen his opinion he quotes many

cases in which the Aurora was seen at one or more of the stations without being visible at Bergen, the headquarters, so to speak, where observations were carefully made both day and night. The phenomenon was only seen three times simultaneously by all the stations comprised in 71 and 55 degrees. And even then, who knows but that it was the same Aurora that each saw?

I cannot close my letter without mentioning why the inauguration of the Berlin electric railroad has been so long delayed. As was feared, electricity escaped from the middle rail, and a copper conductor supported by means of stakes has been substituted. Although this is a great improvement, it is doubtful whether the railway can be used in rainy weather, and this fact justifies an article recently published in *L'Electricité*, which affirms that electric railroads can only be properly employed in tunnels such as, for instance, those of the future Metropolitan in Paris. COSMOS.

**VANILLIN.**—Meissner's new process for the manufacture of the aromatic principle of vanilla consists in producing it from eugenol  $C_{10}H_{12}O_2$  by first forming acetueugenol; then oxidizing the said product with a certain proportion of permanganate of potash in a neutral solution; and, finally, further oxidizing the product with bichromate of potash in a neutral solution. The acetueugenol is obtained by digesting the dry eugenol with an excess of acetylchloride  $C_2H_5OCl$ . The excess of acetylchloride is distilled off, and the remainder used for the production of vanillin. (1.) The crude acetueugenol is oxidized in a neutral solution, 47 to 50 parts of permanganate being used to 20 parts of acetueugenol. The product obtained is separated by a filter press from the binoxide of manganese formed during the oxidation, and after the decomposition of the small quantity of the carbonate of potash by sulphuric acid, the clear liquid is evaporated in a vacuum at 50 deg. C., to about 1-15th of the original volume. The acetvanillin is extracted from the lye thus obtained by repeated agitation with ether. (2.) The lye freed from the acetvanillin is heated to 100 deg. C. to remove all the ether, and after being neutralized is mixed with neutral chromate of potash. The mixture is heated until the chromate is decomposed, and the product filtered off from the oxide of chromium, and shaken up with ether to remove the acetvanillin formed during the oxidizing process. This operation is repeated on the lye several times. After the evaporation of the ether, the acetvanillin is boiled with soda, by which operation crude vanillin is obtained, which is purified by being dissolved in ether, and shaken up in a warm saturated solution of bisulphite of soda, and set aside to crystallize. The crystals are washed in bisulphite of soda solution and strong alcohol, and finally decomposed by sulphuric acid when the vanillin separates as a colorless oil, and can be finally recrystallized in water.

In speaking recently of the Washington telescope we inadvertently referred to it as a 32-inch equatorial. This instrument is well-known to have an object glass of 26 inches diameter. The objectives for the Russian Government ordered by Struve is 30 inches, and the Lick equatorial will have a 36-inch objective.

**THE CAUSE OF SPONTANEOUS DECOMPOSITION OF RAW CANE SUGAR.**—Organisms contained in these sugars multiply and produce an inversive ferment.—U. GAYON.

**PERSISTENT VITALITY OF CARBUNCULAR GERMS, AND THEIR PRESERVATION IN CULTIVATED SOILS.**—At a farm near Senlis, cattle which have died of carbuncular fever twelve years ago have been buried at a certain spot in a walled garden. Samples of the soil were lixiviated and concentrated, and guinea-pigs inoculated with the matter died quickly with well-marked symptoms of carbuncle. Of seven sheep allowed experimentally to pass a few hours daily on this spot, two died of the same disease in the course of six weeks, whilst the rest of the flock from which the seven had been taken remained healthy.—M. PASTEUR.

## BOOKS RECEIVED.

SIR WILLIAM HERSCHEL: HIS LIFE AND WORKS. By EDWARD S. HOLDEN, of the United States Naval Observatory, Washington. Charles Scribner's Sons, 743 Broadway, New York. 1881.

There is a charm which attends the memory of some representative men, and which endears even their history to posterity.

Foremost among such men we recall the name of Herschel, and we could hardly select a more pleasing task than to touch lightly on a few salient points in his eventful career.

In our opinion the great feature in Herschel's history was, that he succeeded in reaching eminence as a scientific man, notwithstanding the apparently insurmountable difficulties that stood in his path to success.

Consider for a moment the position of Herschel when he made his first effort to become an Astronomer. He was 34 years of age, residing in a foreign country where he was unknown, and earning a bare existence as a musician, with a younger brother on his hands, and a sister who was not even acquainted with the language of the country (England) in which they then resided.

They were too poor to hire a servant, and what with out-door performances and giving instruction at home, there was little time for recreation, for even leisure moments were occupied by copying music. So that it was only at night, when he would retire wearily to bed, with a basin of milk, and Smith's *Optics* and Ferguson's *Astronomy*, that he could devote the first thoughts to a science which hereafter must ever be associated with his name.

He would then rise in the morning with thoughts intent on seeing for himself the celestial objects of which he had been reading over night.

To purchase an instrument was out of the question, but with the indomitable energy of will which stamped his career thereafter, he at once determined to make a telescope with his own hands, and not content with striving to see what other observers had observed, he began to contrive a telescope eighteen or twenty feet long.

But to earn an existence by music now occupied every moment, day and night and it was many months before a telescope could be commenced; but finally in 1744, when he was 36 years of age, he completed a Gregorian telescope, and began to view the heavens under circumstances that must have been depressing to a less ardent mind; for he had to contrive a few spare moments as best he could, even running home between the acts at the theatre to make a short observation, and then rushing back to take his position in the band.

And so, with mind divided between the oratorios of the *Messiah*, *Judas Maccabeus*, &c., and the variable star *Mira Ceti*, along with the music went the Astronomy, until on the 13th of March, 1781, Herschel, this amateur astronomical observer of Bath, made one of the most striking discoveries since the invention of the telescope, for in examining the small stars in the neighborhood of *H Geminorum* perceived one which appeared visibly larger than the rest, and this object proved to be the major planet, now called Uranus.

Naturally, this was the turning point of Herschel's life, and his a ter career was a rapid rise to the highest eminence as a scientific man and one of the most accomplished astronomers.

The story of Herschel's life is now presented by Professor Edward S. Holden, in a charming little book which may be read at a single sitting, and yet complete and ample in all the details necessary to convey to the reader a vivid picture of the great Astronomer.

We admire Professor Holden's book for its simplicity of diction; not a superfluous word is given, and most of the more interesting events are given in the very words of his sister as recorded by her.

We desire to see this interesting work in the hands of the youth of this country, for if a noble example of a successful career will stimulate a young man to exalted aspirations for a useful and honorable life, the perusal of the present memoir should have such an inspiring effect.

We acknowledge the receipt of the following important works from the Government of New Zealand, being part of a series prepared by the Colonial Museum and Geological Survey Department, of which James Hector, M. D., C. M. G., F. R. S., is Director in Chief:

A MANUAL OF THE NEW ZEALAND MOLLUSCA.—A systematic and descriptive catalogue of the marine and land shells, and of the soft Mollusks and Polyzoa of New Zealand and the adjacent islands, by Frederick W. Hutton, F. G. S., C. M. Z. C., Professor of Biology, Canterbury College, New Zealand University, Wellington, 1880.

A MANUAL OF THE NEW ZEALAND COLEOPTERA, by Captain Thomas Brown, Wellington, 1880.

This Catalogue occupies 650 pages and contains 1050 species. It is a complete description of all the New Zealand Coleoptera known to Science, classified according to the views of Lacordaire. This valuable work is spoken of as a monument to the zeal and industry of an ardent naturalist.

PALEONTOLOGY OF NEW ZEALAND.—Part IV.—Corals and Bryozoa of the Neozoic period in New Zealand; by the Rev. J. E. Tenison-woods, F. G. S., F. L. S. Wellington, 1880.

The author has a high reputation for his minute acquaintance with the Marine Invertebrata of the tropical and temperate parts of Australia, and during the last twenty years has published many works on the subject, so that the inferences drawn in this work may be received with much confidence.

MANUAL OF THE INDIGENOUS GRASSES OF NEW ZEALAND, by John Buchanan, F. L. S., Land-Botanist and Draughtsman of the Geological Survey. Wellington, 1880.

The general system of classification employed by the author is that adopted from Sir Joseph Hooker's standard works on the New Zealand Flora, but the methods upon which the general and specific characters have been arranged is from a more recent work on the British Flora by the same distinguished botanist. Sixty full-page illustrations are given of specimens, nature-printed, each having, in addition, from 10 to 25 drawings showing the anatomical character of the inflorescence in each species, from original microscopic dissections made by the author, whose excellent botanical knowledge, combined with his skill as a draughtsman, peculiarly fitted him for the work.

TRANSACTIONS AND PROCEEDINGS OF THE NEW ZEALAND INSTITUTE, 1879., Vol. VII., edited by James Hector, C. M. G., M. D., F. R. S.; issued May, 1880. Wellington.

In this volume is a valuable series of papers, many of them well illustrated, and we congratulate the colony on the valuable scientific work accomplished and in progress. We find many of the papers in this volume of the highest interest, and we shall shortly present our readers with selections.

Any of our readers residing in New York who desire to examine these works can do so by calling at our office, and it may be convenient to know that the Colonial Government has arranged a scale of moderate charges, at which any of these publications can be purchased.



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*Pat. Nov. 16, 1869, Oct. 15, 1878.*

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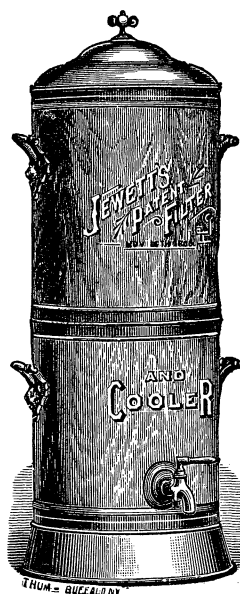
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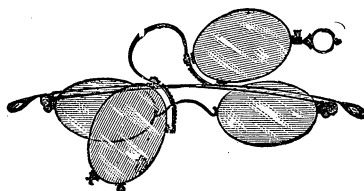
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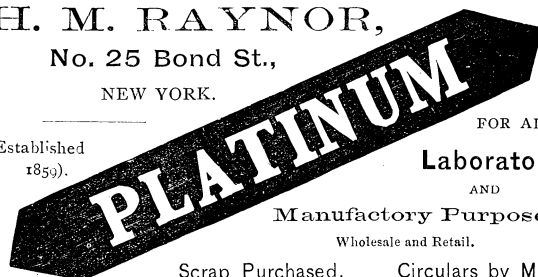
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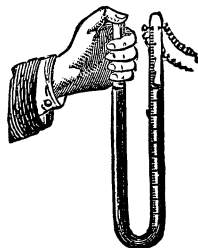
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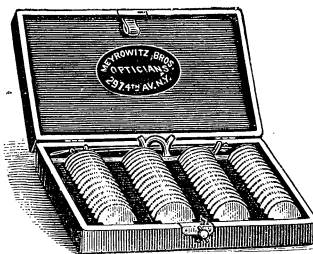


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